ART. XII.—*Leçons de Physiologie Expérimentale, appliquée à la Médecine, faites au College de France.* Par. M. CLAUDE BERNARD, Membre de l'Institut, &c. &c. Tome deuxième. Cours du Semestre d'Été, 1855. Paris, 1856.

Mémoire sur le Pancréas et sur le Rôle du Suc pancréatique dans les phénomènes digestifs. Par M. CLAUDE BERNARD. Paris, 1856.

IT may be truly said that M. Bernard has inaugurated a new epoch in experimental physiology. He has first placed the science on its true footing, and has indicated the only course, that of experimenting upon living or recently killed animals, by which actual and satisfactory progress can be made. He has done the same thing for physiology that Bichat did for anatomy, and that Robin and Verdeil have done for organic chemistry. He has shown us what the science really is, and how it is to be pursued. Experiments upon the living body were, it is true, performed before his time by Magendie and other experimenters; but their attempts were generally awkward and unproductive. They did not comprehend properly the nature of the questions to be settled, nor the manner of accomplisbing their solution; and, consequently, the random and barbarous mutilations to which they resorted, only served to bring discredit upon physiology, without leading to any really valuable acquisitions. Bernard, however, is not only gifted with remarkable dexterity in contriving and performing an experiment, but he has also always before his mind a distinct and definite notion of the object to be accomplished by it, and the mode in which this is to be attained. His experiments, therefore, lead to something. They are the means of progress; and not only settle old points previously in dispute, but constantly suggest new questions, and open new paths of inquiry.

The course of physiology given by M. Bernard at the College of France is somewhat peculiar, also, in another respect. It is not intended to be a systematic course. It does not profess to give a complete and final account of the science, or even of any particular part of it. On the contrary, its especial object is to treat of those portions of physiology which are still incomplete and unsettled; and to present them, so to speak, not in the state of entire maturity, but in that of transition and development. In this respect, a systematic and a progressive course necessarily differ from each other. In a systematio course, such as that given at the Ecole de Médecine and most of our own medical colleges, where the lectures are addressed solely to beginners and undergraduates, it is the object of the lecturer to present to his hearers only the well-established facts of the science, and to inculcate those doctrines which have received the general assent of physiologists. Points which are doubtful or disputed he passes over in silence; since their discussion would only tend to perplex and confuse the mind of the student, who is not yet in a condition to examine and criticize, but only to learn and remember. The science, therefore, as presented in such a series of lectures, has an appearance of completeness and finish, which is very far from belonging to it in reality. All its chasms and deficiencies are concealed, and the attention of the hearer is directed only to those portions which are fully established, and in no danger of being disturbed.

The course at the College of France, however, is entirely different from this, both in its plan and object. Instead of passing over those points which are doubtful and dwelling upon those which are fixed, it passes over those

which are fixed, and dwells upon those which are recent and unsettled. It does not teach, bnt discusses. There the science leaves the position of dogmatism and dignity which it occupies elsewhere, and descends into the arena of the present, in the character of *physiologie militante*—to use an expression of M. Bernard—ready to examine and prove, by experimental means, the questions which may arise upon any physiological doctrine. Bernard carries out this idea to its fullest extent. He desires to show his hearers, not only the results, but the progress, also, of his investigations; and to unfold the manner in which new questions present themselves, and in which they are gradually, and by successive trials, followed to a solution. He does not hesitate, therefore, to present his ideas, frequently, in an incomplete form, even though they may afterward require some modification; for his audience is in this way carried along with him, and the teacher and the student learn together.

There can be no qnestion of the immense value of such a course. It is, indeed, the only means by which the profession can be kept informed of the actual progress of the science; for it is in such a course only that progress is actually made. M. Bernard has, therefore, published his Lectures, not as forming a standard Treatise on Physiology, but as a "simple narration of what takes place in the laboratory of a physiologist who is employed in scientific investigation." The advantage which he expects to derive from this method is a double one. In the first place, it shows the difficulties and complications that surround physiological experiments, and the care which is necessary in performing them; and, in the second place, it is calculated to excite in the minds of the profession an interest which may provoke new and more fruitful investigations; instead of quieting and satisfying them with the knowledge of what has already been done.

Bernard, however, is not blind to the fact that by such a course he lays himself open to innumerable criticisms and attacks. He knows very well, by experience, that for every man who is capable of striking out a new path by himself, there are ten who can follow on his track, and seize eagerly upon any apparent mistake or oversight of his, to throw doubt or discredit upon his conclusions. He cannot resist the temptation, in the preface to his volume for the previons year, of throwing down the gauntlet to these *parasites scientifiques*, as he calls them, and of showing that, while he is conscious of exposing himself to their attacks, he is very willing to undergo that risk for the sake of the advantages which are certain to result to the profession.

> "At the same time," he says, "I am very well aware that a book published on this plan, will afford abundant opportunity for the sterile criticism of those scientific parasites, who, incapable of originating anything themselves, always fasten upon the discoveries of others, and attack them, in order to gain notoriety for themselves. Such a consideration, however, does not deserve even to be thought of, provided I can succeed in my object, and make this course useful to those who try to extract what is good from the labours of others, in order to draw from it a fresh stimulus for the improvement and furtherance of scientific pursuits."

The former volnme published by Bernard contained his lectnres, for the winter course of 1854–55, on the Formation and Destruction of Sugar in the Animal Body. The present volume contains his lectures, delivered in the summer course of 1855, on the Character and Functions of the Digestive Fluids. It must not be supposed, from what has been said of the experimental and progressive character of Bernard's lectures, that he is destitute of philosophical ideas, or incapable of taking enlarged views in physiology. On

the contrary, his lectures are always arranged in accordance with such philosophical ideas as show him to be a real physiologist, in the full meaning of the term, and not a simple experimenter. In the first chapter of the present volume, he discusses the proper mode of investigating a physiological question, together with the point of departure at which the investigation should commence in order to arrive with certainty at its object; and more particularly the relative positions, in this respect, of anatomy and physiology.

Anatomy has too often been regarded, not only as an introduction to physiology, but even as a legitimate means by which discoveries in physiology might be effected. When the compound microscope became so far improved that it enabled us to study in detail the minute cells and fibres of the body, by how many was it anticipated that this minute knowledge of the structure of parts would give us, at the same time, a definite idea of their functions? No such knowledge, however, was obtained from microscopic anatomy; and there are yet many persons who do not fairly understand how it happens that we were disappointed in this particular.

Bernard puts the question upon its true footing, when he says that there is no such relation existing between anatomy and physiology that one can be used as a means of learning the other. There is no possibility of deducing a physiological fact from an anatomical fact—the function of a part from its anatomical structure. A knowledge of anatomy is certainly essential to the physiologist, but only as a preparation, in order that he may be able to experiment successfully, and use his instruments without bungling. Anatomy does not really give him any information as to the mode of action of an organ, but only as to its size, form, and structure. A little confusion, however, is apt to exist, in all minds, on this point, in regard to those organs whose function is purely mechanical; as, for example, the urinary bladder or the arteries. We are apt to think that we deduce from the shape of the bladder its function as a containing sac or reservoir, and from that of the arteries their function as conveying tubes or ducts. But, as M. Bernard very justly remarks, we do not really make the deduction, even in this case; but are only enabled to guess at the function of these parts, because they *resemble, in their figure, tubes and ducts which we have seen and used elsewhere,* as, for example, in the mechanical arts, and the functions of which we have already learned experimentally. But so soon as anatomy presents to us certain organic forms, such as nervous and muscular fibres, for which we can find no analogies in our previous experience, we are totally at a loss, and can make no progress without resorting to direct experiment or observation. A very simple but striking comparison will serve to illustrate the justice of these remarks.

"If we were to enter a workshop," says M. Bernard (page 7), "in which some mechanical operation was carried on, the details of which were unknown to us, examine the tools and instruments as much as we pleased, we should never be able to guess at the use for which they were designed, without seeing them in operation. We can only comprehend the action of the different pieces of mechanism by seeing the machine at work. Then we at once understand their figure and relations; and, in connection with their observed uses, can easily remember the forms, in which we before saw nothing but confused and inexplicable details."

It is very plain, then, that there is no basis for the idea that physiological facts may, in any case, be deduced from anatomy; and as forms alone are incapable of indicating to us the function of a part, so two organs may have the same anatomical form, and yet differ widely in their functions. This truth furnishes the text for a considerable portion of the present volume. It is

illustrated in the case of two glandular apparatuses, viz., the pancreas and the salivary glands. These organs have been considered as similar in their functions, because they resemble each other in their anatomy. Depending upon the external and gross anatomical characters, the old anatomists regarded the pancreas as an "abdominal salivary gland;" and the name by which it is still designated among the Germans, *bauch-speichel-drüse,* indicates the same supposed resemblance. Even in their minute structure an equal similarity exists between them; and microscopic examination fails to distinguish the elements of the parotid gland from those of the submaxillary, or those of the submaxillary from those of the sublingual. Nevertheless, each of these organs is different in function from all the others, and cannot be replaced by them; for though the form of their cells and follicles is the same, the fluids which they secrete have different ingredients and are destined for different purposes.

Taking these principles for his guide, Bernard adopts a mode of investigation which he terms the *physiological* or *functional* method, in contradistinction to the anatomical or organic. This distinction is, in reality, of some importance; and has, like the others already mentioned, a practical bearing. Physiology has for the most part been studied from an anatomical point of view. That is, the problem which the investigator proposes to himself is as follows: a particular organ being given, to discover its function. We ask ourselves, what is the function of the spleen, the thyroid body, the supra-renal capsules? That is, we start with the organ; and by experimenting on that, we endeavour finally to arrive at its function.

Bernard takes the opposite course. He starts with the function, or physiological phenomenon, and endeavours by his experiments to determine the organ or organs which are concerned in its production. For example, in his investigations on the production of sugar in the circulation, his attention was first directed to the physiological fact that sugar makes its appearance in the living body; and it was only afterward that he was led to the discovery that this production takes place in the liver. This he calls the physiological method, because it is the function itself which is the first object of examination; and its localization, or connection with a particular organ, is a final result, at which he arrives subsequently. There can be no question that this is the true method, if for no other reason than that several organs sometimes combine to perform a single function, and on the other hand a single organ sometimes performs a complex function. Thus the movement of the blood in the arteries is a complex motion, resulting from the simultaneous action of the contractions of the heart and the elasticity of the arteries; and again the liver, which is a single organ, furnishes two secretions, the bile and the liver-sugar; which pass off in different directions, and are intended for different purposes.

In order to have a just idea of physiological phenomena, we must remember their mutual connection. They cannot be taken apart from each other, like the pieces of a Chinese puzzle, and studied separately; or, if we do so, it is only for the purpose of temporary convenience, and in order that we may understand them better in connection. Respiration, in the living body, is inseparably connected with excretion and digestion, the latter with secretion and absorption. One cannot go on without the other; and it is because we have sometimes made the mistake of regarding them as independent processes, that we have been led insensibly to adopt false views. Thus, in respiration, seeing oxygen inhaled and carbonic acid expired, we have regarded this as the whole history of the matter, and have supposed that the oxygen combined directly with the carbon of the blood to form carbonic acid. Further exa-

mination has shown us that the phenomenon is more complicated than this, and that the appearance of carbonic acid in the different tissues of the body is a part of many nutritive and excretory changes, which are no more closely connected with the absorption of oxygen than they are with other phenomena; those of digestion, for example, and metamorphosis. The function of respiration, therefore, in its true sense, is by no means localized in the lungs, or even in the blood, but is carried on throughout the entire body; its details, moreover, varying considerably in different organs, and being everywhere closely connected with other processes.

It is really of importance, therefore, that we should keep in mind this connection which exists between the different functions; and more particularly the manner in which the various organs unite in producing the phenomena of life. This connection cannot be better expressed than it is by Mr. Bernard himself.

"In tho anatomical method," he says (page 15), "we take the organs one after another, and ask ourselves of each one, What is its use?

"But though we can dissect apart all the different organs of the body after death, and isolate them from each other so as to study their form, their structure, and their relations, it is not possible to do so during life, while all these parts are acting in combination to produce a common effect. An organ does not live by itself. We may almost say, even, that it has no separate anatomical existence, for the limits which have been assigned to it are often, in this respect, purely arbitrary. It is only the entire organism that lives and acts. We can never get an idea of the action of any kind of mechanism by studying its separate pieces one after the other. So in studying physiology by a purely anatomical method, we may take the organism to pieces, but we cannot in this way learn the combined operation of the whole. This we can do only by observing tho organs whilo they are in a state of activity."

The changes which the food undergoes in the process of digestion are of two kinds, physical and chemical. The physical changes—that is, mechanical division, trituration and mastication—are intended simply to prepare the food for the subsequent chemical operation of the various digestive fluids. The author remarks, in this connection, that the instincts of animals, which lead them to prefer particular kinds of food, are determined almost altogether by the physical properties of the alimentary substances, and not by their chemical constitution. The carnivorous species prefer animal food because their masticating apparatus is not calculated for the disintegration of hard vegetable substances, such as grains, stalks, nuts, &c.; while the vegetable-feeders have no organs suitable for the seizure of living prey or the laceration of animal flesh. The entire organism of the carnivora and herbivora is consequently adapted to the prehension as well as to the mastication of particular kinds of food; the carnivora being active, fierce, and crafty, while the herbivora are comparatively sluggish, timid, and simple. The organs of mastication, accordingly, which are regulated by the varying physical qualities of animal flesh, grass, grains, herbage, roots, fruits, &c., vary exceedingly in different animals. But the chemical constitution of all alimentary substances, whether animal or vegetable, is nearly the same. The albuminoid, oily, and saccharine elements present similar characters and reactions, from whatever source they may be derived. Consequently the digestive fluids and their chemical properties and actions are the same in both the carnivorous and herbivorous species. A carnivorous animal may accordingly be induced to use vegetable food if its physical properties be artificially modified so as to become adapted to his organs of mastication. A dog, for example, as the author remarks, will refuse to feed upon wheat or rye in the grain, and will even starve to death

with a supply of such food before him. But if it be ground and made into bread, he will then take it freely, and even subsist on it for a considerable time. On this account, it follows that the comparative physiology of digestion is more easily pursued; since there is less variation in the properties of the digestive fluids in different animals than might be anticipated.

In the second chapter, the author gives the literary history of the different *salivary glands.* He shows that, owing to the identity of their anatomical structure, they have usually been considered as similar in their functions. Bernard, however, has for some years pointed out, in his public courses, the very different properties of their secreted fluids. The parotid saliva, the submaxillary, the sublingual, and the secretion of the mucous follicles of the mouth, all have their distinctive characters, varying principally in the degree of their viscidity, and the nervous influences which regulate their secretion. The parotid saliva, for example, is excited by anything which puts in action the masticatory apparatus; the submaxillary by irritation of the lingual nerve, and by the contact of sapid substances; while the sublingual saliva and the buccal mucus are required mostly to assist in the process of deglutition.

The saliva, however, as a digestive agent, is a complex fluid resulting from the mixture of all the different secretions from the above glands. It consists of water holding in solution the various animal matters known as ptyaline, mucus, extractive, &c., with alkaline carbonates, chlorides, sulphates, lactates, earthy phosphates, and a minute quantity of sulpho-cyanide of potassium. With regard to the last named substance, Bernard is disposed to believe that it is not a necessary nor constant ingredient of the saliva. He has sometimes found it present, and sometimes absent, in cases where there was no evidence of any morbid condition of the system. When present, it appears to be mostly due to an alteration of the organic elements of the saliva; and the author is inclined to regard it as dependent, in some way, on caries of the teeth, since he has not observed it in persons whose teeth were entirely sound. If so, it should be considered as an accidental product; and in any case it appears to be of little or no importance, so far as regards the physiological properties of the saliva.

Some very interesting experiments are related on the *excretion of medicinal substances through the salivary glands.* Iodide of potassium, for example, introduced into the circulation, made its appearance in the parotid and submaxillary saliva almost instantaneously, while it appeared in the urine only after three hours. Ferrocyanide of potassium, on the other hand, injected into the bloodvessels, appeared in the urine in seven minutes, while no trace of it could be detected in the saliva, even at the end of four hours. These two substances, therefore, though both soluble in the animal fluids, and generally diffused through the circulation, pass out by the various glandular organs with very different degrees of facility. Another not less remarkable fact observed by the author, is that the same substance, introduced into the blood, will continue to appear in one secretion longer than in another. Thirty grains of iodide of potassium were introduced into the stomach of a dog, through a gastric fistula; and shortly afterwards the iodine made its appearance in both the urine and saliva. The next day it had disappeared from the urine, so that it might have been supposed to be entirely eliminated. In point of fact, however, it was still present in the saliva, and *continued to show itself there for a period of three weeks.* This was the more remarkable, since it will be remembered that this substance, after being introduced into the system, appears in the urine more readily than in the saliva, and that the time of its appearance in the urine varies with the quantity ingested; a large

dose appearing rapidly, and a small one requiring a longer interval. It is evident that the exact mechanism of these eliminations is not entirely understood.

The mechanism of the secretion of the saliva itself is also, to a certain extent, difficult of explanation. Those who like to simplify as much as possible all the vital phenomena, and reduce them to the exclusive operation of ordinary physical forces, have attempted to explain the passage of the secreted fluids through the glandular tissues by referring it to the simple effect of pressure in the bloodvessels, by which varying quantities of water, saline ingredients, and animal matters would pass out by exosmosis, in proportion to the amount of pressure exerted. This explanation, however, will not withstand the test of investigation. The author refers to some experiments of Ludwig's, in which he found, by applying his mercurial gauges at the same time to the duct of Steno and the artery of the parotid, that the pressure on the duct, from the secreted saliva, was considerably greater than that in the artery; so that the passage of the secreted fluids had really taken place in a direction contrary to that which would have been caused by the simple influence of pressure. Bernard himself states other facts, equally striking in this respect. He found, for example, that ferro-cyanide of potassium, which is not excreted by the parotid glands, is, however, readily absorbed by them, and if injected into the duct of Steno, will make its appearance in the urine; while, at the same time, it is not discharged with the saliva even of the gland into which it has been injected. All these facts show that the active agents in the process of elimination are the secreting cells of the gland itself; and that they have the property of absorbing particular substances, and of causing them to transude in a particular direction.

The author enters at length into the consideration of the *digestive properties of the saliva.* These he shows, however, to be much more of a physical than of a chemical nature. He does not, indeed, deny the property, which has been attributed to the saliva, of converting boiled starch into sugar. On the contrary, he describes it in detail, and gives many experiments of his own which were made with a view of ascertaining to which of the salivary secretions this properly belongs. He shows, however, that this action on starch does not belong to the fresh secretion taken directly from the parotid, submaxillary, or sublingual ducts, but only to the mixed fluids of the mouth which have begun to undergo a putrefactive change. Furthermore, it is not a property peculiar to the saliva, but exists also in various other animal fluids, morbid as well as normal, such as peritoneal and pleuritic effusions, mucous discharges from the bladder and rectum, infusions of mucous membrane, &c., which have been for a short time exposed to the contact of the air. These facts, it is true, are not incompatible with the supposition that the mixed saliva of the mouth may yet be intended normally for the digestion of starch. That question can only be settled by direct experiment on the living animal, after the ingestion of amylaceous substances; and the result of such experiments shows conclusively that though saliva, mixed in a test-tube with boiled starch, converts it into sugar, it does not have this effect when mingled naturally with the food in mastication and swallowed into the stomach.

"Consequently," he says (p. 158), "although one might be supposed to believe that, in the human subject, the saliva may transform starch into sugar during its passage through the mouth, on account of its remarkable activity as a mixed fluid, there are several reasons why we cannot admit this to be the case in the lower animals. In the first place, saliva cannot exert this action on raw starch, but only on that which has been boiled or *hydrated;* and ani-

mals do not usually take any amylaceous substances in this form. Secondly, the transforming influence of the saliva can only be exerted between the mouth and the stomach; since the food, when it has once entered the stomach, is no longer under favourable conditions to be acted on by the saliva, owing to the presence of the gastric juice, which interferes with it. We shall see presently what is the origin of this peculiar property which belongs to the mixed saliva, but not to either of the salivary fluids taken separately. What I wish to say here is that it is to be regarded, not as an essential, but rather as an accidental property of the secretion; since its action in the lower animals is extremely feeble, and even insignificant in the normal condition of the digestive apparatus. For if we give a dog even cooked starch with his food, it is found afterward in the stomach without having undergone any sensible modification."

These conclusions, to which Bernard has been conducted by direct experiment, have been corroborated by other observers, and are undoubtedly correct. They show how important it is to remember that the digestive process, though carried on by different digestive fluids, is a continuous process, in which these fluids become mingled together, and modify, to some extent, the action which each one would exert separately. The author shows, also, in a subsequent chapter, how starchy matters are really digested in the living animal, viz., by the intestinal fluids, which act upon them only after they have left the stomach and passed into the duodenum; these fluids, the most active of which is the pancreatic juice, being much more efficient than the saliva, and exerting a transforming influence on starch, not only in a hydrated, but also in its natural condition.

The real physiological action of the saliva, as already intimated, is a physical one. It is deduced from experiments which show that the quantity of the secretion which is poured out and mixed with the food, is regulated entirely by the physical condition of the alimentary substances. The mode by which this fact was ascertained is as follows: An animal of large size was taken, a ligature placed upon the œsophagus at the lower part of the neck, and an opening made into its cavity above the ligature. Various articles of food, previously weighed, were then administered to the animal, and, as they passed out at the œsophageal wound, were again collected and weighed afresh. The difference in weight showed, of course, the quantity of saliva which each one had absorbed during the processes of mastication and swallowing. One of the tables given by the author will be sufficient to show the nature of the results which were obtained.

Kind of food.	Weight before mastication.	Weight after mastication.	Difference; showing quantity of saliva.	Name of experimenter.
	Grammes.	Grammes.	Grammes.	
Straw	20	100	80	Lassaigne.
Hay	325	2000	1675	Hygienic Commission.
Hay	20	91	71	Lassaigne.
Oats	520	1168	648	Hygienic Commission.
Oats	46	100	54	Lassaigne.
Fecula and bran .	250	725	475	Hygienic Commission.
Barley meal . .	31	100	69	Lassaigne.
Green barley leaves and stalks . .	67	100	33	
250 grammes fecula and bran, with 1000 grammes of water .	1250	1256	6	Bernard.

It will be seen, by glancing at the above, that those substances which require the largest quantity of saliva are dry stalks, hay, bran, &c. Green

leaves and stalks require much less; while bran and starchy matters, already sufficiently moistened with water, absorb only about two and a half per cent. of their weight while passing through the mouth and œsophagus.

The principal object of this secretion is, therefore, to moisten and soften the refractory kinds of food, and to prepare them for the action of those digestive fluids with which they are afterward to come in contact. Its importance in assisting the mastication of hard substances is well shown in one of Bernard's experiments in which he administered to a horse about one pound (500 grammes) of oats, which were easily masticated and swallowed by the animal in nine minutes. Bernard then divided both parotid ducts, so that the saliva ran away from the external wound instead of passing into the mouth. A similar quantity of oats being again administered to the animal, mastication went on with the greatest difficulty; so that at the end of twenty-five minutes nearly a quarter part of the grain still remained uneaten. The œsophagus had been tied and opened at the commencement of the experiment, and none of the first pound of oats had passed into the stomach. The difficulty and delay in masticating the second pound were not, therefore, owing to the animal's appetite having been satisfied, but merely to the absence of the parotid saliva.

The author finishes this part of the subject with the following *resumé* of the digestive properties of the saliva:—

> "Finally," (page 167) "the saliva appears to exert little or no chemical action on the digestive process. If we follow, in fact, the starchy elements of the food through the alimentary canal, particularly where they are taken in a raw state, we find that they disappear and are transformed into sugar only in the small intestine, and by the action of other fluids than the saliva.
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> "The real action of the salivary fluids is, as the ancients formerly believed, purely physical in its character, and is subservient merely to mastication, taste, and deglutition. The experimental evidences in favour of this conclusion are of the most decisive character."

The above opinion, as to the digestive action of the saliva, corresponds essentially with that of most others. Bidder and Schmidt more particularly (*Verdauungs-Saefte und Stoff-wechsel*, Leipsig, 1852), who were at first very much inclined to regard this secretion as intended for the digestion of starch, have finally been led to conclusions similar to those of Bernard. They also have satisfied themselves that no sugar is to be found in the stomach of the living animal after feeding on starchy substances; the small quantity which may be produced by contact with the saliva in the mouth, or œsophagus, being immediately absorbed, according to them, or converted into lactic acid, and no further transformation of the starch taking place so long as it remains in the stomach. They believe, too, that the saliva is destined principally for the moistening and reduction of hard and dry alimentary substances, and is also subservient to what they call the internal circulation, or "interchange of fluids within the animal organism."

There is one very interesting question regarding the saliva, which we could wish had been more fully treated by the author, viz., that of the *entire quantity of fluid secreted* by the different salivary glands. Such an estimate has in fact been made by Bidder and Schmidt, and is referred to by the author; but it seems to us that he rather undervalues their conclusions. It is very true, as Bernard observes, that the quantity of secreted fluid varies much from time to time, according to the period of the day, and the quantity and quality of the food. All the secretions, indeed, are liable to such a variation, and any estimate of their total quantity, which pretends to be absolutely exact,

can only give rise to mistaken ideas in physiology. But the same thing may be said of their quantitative analysis. The relative quantities of water, chloride of sodium, animal matters, etc., not only in the saliva, but also in the blood, gastric juice, urine, and all animal fluids, vary, within certain limits, every hour, and almost every minute. Such a variation is characteristic of the mixed fluids of the living organism, and separates them by a wide interval from inorganic substances of definite chemical composition. But that does not prevent us from giving always, in books on physiology, the quantitative analysis of the blood, urine, saliva, etc.; only we understand always that such analysis is approximative merely, and not exact or constant. There can be no doubt that the different secretions are poured out every day in quantities which vary within certain limits, according to circumstances, but which are still capable of an average estimate.

The methods adopted by Bidder and Schmidt, with regard to the saliva, were as follows:—

They found that, in a dog weighing thirty-four pounds, they could obtain in one hour, from a single submaxillary duct, 84.6 grains of saliva; so that both submaxillary glands would furnish, during the same time, 179.2 grains. From a single parotid duct they obtained, in one hour, 131.95 grains; so that both parotids would give, per hour, 263.90 grains. These results, applied to a man weighing a hundred and forty pounds, would give, during twenty-four hours, 16,230 grains for both submaxillaries, and 25,305 grains for both parotids. The authors reduce this estimate, however, one-half, in order to allow for the unnatural stimulus which may have operated on the dog while under experiment; and so come to the conclusion that the entire daily quantity, for the human subject, is not less than 20,768 grains, or $2\frac{97}{100}$ pounds. Direct experiment on their own persons gave even a larger quantity than this. The experimenter found that by collecting all his own saliva, without applying any unnatural stimulus, but simply taking care that none of the salivary fluid was swallowed, from 1,500 to 1,800 grains were secreted in the course of an hour. This would give for the whole day (subtracting seven hours for sleep) fully 23,000 grains, or a little over three pounds and a quarter for the entire quantity of saliva in the human subject. It is evident that although this quantity cannot be regarded as altogether exact, it is certainly not overestimated; and it shows that the saliva, as well as most of the animal fluids, are really secreted in much greater abundance than was formerly supposed.

From the saliva, the author passes on to the consideration of the *pancreatic juice.* Indeed, it is one of the principal objects of his course to institute a comparison between the pancreatic and salivary fluids; and to show that, notwithstanding the apparent identity in anatomical structure between the glands, the fluids secreted by them are entirely different in their physiological properties. He also illustrates, in a very striking manner, the "physiological method" of investigation, as he calls it, which we have already alluded to in a former part of this article, by detailing the mode in which he was led to discover the function of the pancreatic juice. It was, as he says, while studying the digestion of different substances in the alimentary canal, that he observed that *oily* matters were unchanged so long as they remained in the stomach, and could even be recognized for a short distance from the pylorus in the small intestine. Soon afterward, however, they disappeared, became changed by digestion into chyle, and filled the lacteals with a white milky emulsion. The place where this digestion took place, however, varied in different animals. For while in the rabbit the chyliferous vessels became filled only at the distance of a foot or more from the pylorus, in the dog they

commenced to show themselves almost immediately below the pyloric orifice. On searching for some anatomical cause for this difference, Bernard found that the pancreatic duct opened differently in these animals; in the dog near the pylorus, and in the rabbit twelve or fourteen inches lower down. The alteration of the fats in the intestine, therefore, corresponded in place with the opening of the pancreatic duct, and pointed to the secretion of this gland as the active agent in their digestion. It was afterward shown, by placing the two substances in contact with each other, that the pancreatic juice did really exert a decisive action on fat; and one of its most important functions was in this way finally ascertained.

Before giving the physical and chemical properties of the pancreatic juice, Bernard points out an anatomical peculiarity of the gland, which has been altogether ignored by anatomists, or too lightly passed over; that is, the existence of two pancreatic ducts, both in the human subject and most quadrupeds, such as the dog, cat, horse, etc., opening into the intestine within an inch or so of each other. This arrangement, which has usually been considered as an accidental irregularity, is asserted by the author to be the constant and normal disposition of the parts. In the rabbit alone, though two ducts exist, the upper is usually so minute as not to be of any practical importance. The above fact, of course, is very essential to be known in operating on the pancreas for purposes of experiment, and its neglect has sometimes given rise, as the author afterward points out, to serious physiological errors.

The physical and chemical properties of the pancreatic juice are given by the author in detail. It is a clear, colourless, viscid, alkaline fluid, coagulating completely by the application of heat, and also by admixture with alcohol and sulphate of magnesia in excess. Its composition, according to Bernard, is as follows:—

Water	90 to 92 per cent.
Solids	10 to 8 " "

The solid matters consist of—

An animal substance, coagulable by alcohol, and containing always lime in combination . .	90 to 92 per cent.
Carbonate of soda Chloride of sodium Chloride of potassium Chloride of lime }	10 to 8 per cent.

One of the most remarkable properties of this secretion, and one that had been previously noticed by Tiedemann and Gmelin, is that of *giving a red colour on the addition of chlorine.* This reaction, Bernard finds, does not belong to the pancreatic juice in its perfectly fresh condition, but only after it has become slightly altered by putrefaction. Still, as no similar appearance is manifested by any other of the gastric or intestinal fluids at any period of their decomposition, it may be used to advantage in detecting the presence of pancreatic juice in any part of the alimentary canal.

Another property, still more characteristic, belonging to this fluid, is that of emulsifying and acidifying fatty substances. This property, which also belongs to the substance of the pancreas, is used by Bernard as a decisive test for the presence either of the pancreatic juice or the tissue of the gland. His mode of applying it is as follows: He takes a small quantity of the suspected fluid or tissue, and places it upon a slip of glass together with a little

perfectly neutral fat or oil, and a strong solution of litmus, and then covers the mixture with another glass slip. In a short time, provided the whole be kept at the temperature of eighty to a hundred degrees F., the litmus solution begins to turn red, and its colour is soon entirely changed. The oily matter is decomposed with the production of a fatty acid, which is readily detected by its reaction with the litmus. This property is regarded by Bernard as so thoroughly peculiar that he relies on it entirely for the detection and recognition of the pancreatic tissue, wherever it may present itself, and under whatever different forms. Thus he finds that the duodenal glandulæ, which were regarded by Brunner as similar in character to the pancreas, are in reality entirely destitute of this property, and have no real physiological connection with the pancreas. The cæcal tubes which surround the upper part of the duodenum in certain fish, and which have been regarded as an elementary or incomplete pancreas, are also destitute of any such action on fatty substances; while other glandular organs of a different shape may be recognized by the same test as really supplying the place of the pancreas of the higher animals.

The author's statements, however, with regard to the acidifying influence exerted by the pancreatic juice on fats, have been to a certain extent misunderstood. Bernard recognizes two different actions exerted by the pancreatic juice on oil, when the two fluids are mixed in a test-tube. First, a physical action, by which the oil is reduced to a state of minute subdivision, or *emulsioned;* this effect is instantaneous. Secondly, a chemical action, by which the oil is *decomposed,* with the liberation of a fatty acid and glycerine; this effect requires a certain time for its accomplishment. It has been represented that Bernard regarded both these effects as taking place during the natural process of digestion; an opinion easily shown to be erroneous. But no one understands better than our author that an action which takes place with an animal fluid in a test-tube, does not always show itself in the same way in the interior of the living body; and that direct experiment is the only way of decisively solving the question. He speaks of the acidifying action of the pancreatic juice only as a property which may be used as a test for that fluid, and as only important in this respect. Of the two different actions, mentioned above, which it exerts on fat, the first alone takes place in the natural process of digestion. The fat is emulsioned in the intestine, but it is not decomposed.

"We will now recur," he says (page 320), "to the question which presented itself to us at the commencement of this chapter; that is, whether oleaginous matters are really decomposed during digestion into a fatty acid and glycerine, and whether certain ingredients of the pancreatic juice are absorbed in company with them. With that view, we must examine the condition of the fat as it exists in the chyle; that is, after its absorption, so as to ascertain the chemical modifications which it may have suffered.

"When we examine the oleaginous matter of the chyle, we always find it in a state of minute subdivision, but perfectly recognizable under the microscope by its physical characters. But it is impossible to detect in it, by chemical means, the existence of glycerine or a free fatty acid. So that the fat, as it exists in the chyle, has been physically modified, but does not seem to have undergone any chemical alteration. Even if we introduce into the intestine certain fatty acids, such as the oleic acid of commerce, we cannot afterward detect their presence in the chyle."

Bernard, therefore, determines the pancreatic juice to be the active agent in the digestion of the fatty ingredients of the food. He rests this conclusion upon several different facts. First, the pancreatic juice, mixed with oil in a test-tube, instantly makes a complete and permanent emulsion; an effect which is produced by no other digestive fluid. Secondly, fat is actually emulsioned

in the intestine during life, and is absorbed in this condition by the lacteals. Thirdly, this emulsion of fat and the milky appearance of the lacteals occur in the intestine only at or below the orifice of the pancreatic duct. Fourthly, when the pancreas is injured or atrophied, by the injection into its tissue of foreign matters, fatty substances taken with the food pass out unchanged with the feces; and, fifthly, various instances are recorded, in the human subject, where undigested fatty substances have been discharged with the stools, and in which a more or less complete alteration of the pancreas has been found to exist at the *post-mortem* examination.

One of the most interesting portions of the book is the thirteenth chapter, which is devoted by M. Bernard to a critical review of the various objections which have been made against his doctrine of the digestive action of the pancreatic juice, and to a refutation of them. So novel and important a discovery as that which he announced some years ago, of the action of the pancreatic fluid on fatty matters, could not be received without a very close and searching examination; and the objections which have been made to it from time to time have been very numerous, and, in some instances, very important and positive. The author, however, does not yield in the present volume any important point, but defends his doctrine with the greatest confidence and ability. Some writers have even disputed the physical properties which he attributed to the secretion. They have denied that it was coagulable by heat; and that it possessed any emulsifying power on fat, superior to that of the bile, saliva, and other animal fluids. Bernard shows in what manner they have been deceived. The *fresh* juice, taken within a few hours after the opening of the pancreatic duct, and while the neighbouring parts are still free from inflammation, he asserts to be always moderate in quantity, viscid, and coagulable by heat as completely as the white of egg. In this state, the emulsion which it makes with fat is complete and permanent, while that made with bile or saliva is incomplete and soon separates into its oily and serous portions. But if the animal be in an unhealthy condition, or if the fluid be drawn after irritation and peritonitis have been set up about the pancreas, the secretion is then more abundant than natural, thinner, but slightly coagulable, and incapable of acting with energy on fatty substances. It is now generally conceded, we believe, that he is right in this particular. The pancreatic juice, obtained according to his directions from the healthy animal, does certainly, in many instances, possess all the properties of coagulability and emulsifying power which he ascribed to it; and it seems altogether probable that those writers who have denied their existence have been misled by experimenting upon an unnatural and depraved secretion.

Frerichs and Lenz, furthermore, have undertaken to show that fatty matters may be digested and the lacteals become filled with chyle, after the pancreatic juice has been excluded from the intestine. For this purpose, they tied the pancreatic duct in cats, and afterwards, feeding the animals with fatty substances, found the lacteals well filled with milky chyle. In some instances, they tied the intestine below the pancreas and then injected into its lower portion oily fluids and milk; after which they also found the fatty matters absorbed, and the lacteal vessels filled. Bernard points out the very singular defect in this latter experiment, that the milk, which the observers injected into the intestine, was itself an oily emulsion, and would, therefore, naturally be taken up by the lacteals without the assistance of any further digestion As to the former observation, in which a ligature was placed upon the pancreatic duct, he rejects it altogether as a conclusive experiment, since in cats, as in dogs and in the human subject, there are normally two pancreatic ducts,

freely communicating with each other; a fact of which the experimenters seem to have been ignorant. Bernard gives several drawings of these double pancreatic ducts in the cat, showing their communication with each other, and the varieties of their disposition. One of these ducts alone having been tied, the secretion could readily escape by the other. The pancreatic juice was not, therefore, as the experimenters supposed, excluded from the intestine, and the fat was of course digested and absorbed as usual.

One of the most important of Bernard's experiments, however, is that which he performs on the rabbit, and in which he shows that the digestion and absorption of fat in the intestine corresponds with the orifice of the pancreatic duct. In this animal, the biliary duct opens as usual just below the pylorus, but the pancreatic duct, instead of entering the intestine in company with the biliary, opens some twelve or fourteen inches lower down. Bernard's experiment consists in feeding the animal with fatty substances and then killing him; after which he finds that there are no milky lacteals distributed upon the intestine between the biliary and pancreatic ducts, but that they immediately become abundant below the level of the latter. Bidder and Schmidt made the following remarkable objection to this experiment. They say that Bernard killed his animals three or four hours after feeding, and just at the time that the fatty matters had all been evacuated from the stomach, and carried down the intestine to the point of opening of the pancreatic duct, and that it was simply on this account that he found lacteals below this point and not above it; but that if the animals had been killed at an earlier period, say one hour after feeding, lacteals would have been found also at a higher level, and above the point of opening of the pancreatic duct.

This explanation, however, is rejected by the author as destitute of foundation.

> "I have often satisfied myself," he says (p. 344), "of its incorrectness, by continuing to feed the animals with fatty substances at short intervals, so as to keep a constant supply of fat passing into the system. We can then see, notwithstanding the stomach and duodenum both contain fat, that this substance is fully emulsioned only below the level of the pancreatic duct."

In this manner he refutes the objections of his opponents, and maintains his ground on almost every point in dispute.

The author, however, does not regard the digestive action of the pancreatic juice as confined to fatty substances. On the contrary, he shows that it is also the active agent in the solution and saccharification of starch. Amylaceous matters, when subjected to its action, are speedily disintegrated and converted into sugar.

> "The action of the pancreatic juice on starch," he says (p. 333), "is shown more directly still in the living body. If we feed a dog with boiled amylaceous matters, and afterwards examine the contents of the alimentary canal, we find that in the stomach the starch is still unaltered, since it strikes a blue colour with iodine, and does not reduce the salts of copper; while in the duodenum it is no longer recognizable as starch, but gives the reaction of sugar immediately after it has come in contact with the pancreatic juice."

Bernard maintains that the digestive properties of this secretion are not exhausted by the starchy and oily elements of the food; but are exerted also in a very essential manner on the azotized or albuminoid substances. These matters, according to him, are mostly dissolved by the gastric juice; but this solution is neutralized and precipitated on coming in contact with the bile, so that all the albuminoid elements of the food must be again dissolved before they can be absorbed. The agent of their re-solution he believes to be the

pancreatic juice. He attributes to this secretion, therefore, what will be considered by many an excessive importance in the digestive process. He regards it, indeed, as he himself says, as the *most active* agent of all the digestive fluids; exerting an essential influence upon all the different alimentary substances—oily, amylaceous, and albuminoid—and preparing them all for their final absorption from the cavity of the intestine. His theory of the very intricate changes which take place in intestinal digestion is too complicated and incomplete to be presented here. In fact, there are certain statements of his, with regard to this part of the digestive process, which do not correspond with those of other observers, and which must still be regarded as of very uncertain value. It must be acknowledged that there are yet many points in intestinal digestion which are, to a great extent, involved in obscurity. We can easily obtain separately the gastric juice, bile, and pancreatic fluid, and examine artificially their action on the different elements of the food. But how these actions are modified, or what new actions are set up in the interior of the intestine, when these fluids are mingled together and operate upon a mixture of half-digested oily, starchy, and azotized substances, is a question which will require for its complete solution more laborious and persevering study than has yet been devoted to it. The influence of the bile alone, and the modifications which this important fluid itself suffers in the intestine, are still but very imperfectly understood; and the suggestions of M. Bernard with regard to it, in the present treatise, are too vague and general to give any definite satisfaction on this point. We shall, therefore, pass over his account of the successive action of the intestinal fluids, remarking only that he is evidently aware of the difficult nature of the subject, and conscious of the deficiencies under which we labour in regard to it.

The last chapter of the book is occupied with some exceedingly just and philosophical observations on the processes of nutrition in general. The author points out the erroneous nature of certain opinions which have been prevalent with regard to this subject, and shows how they must be modified. Nutrition, for example, has been regarded as a process by which the different proximate principles, albumen, sugar, oil, &c., are simply absorbed from without, suffering only a kind of solution or liquefaction in the intestine, and *transported*, so to speak, directly into the interior of the body; so that the albumen, fat, and sugar of the food become the albumen, fat, and sugar of the body. This is called "direct" nutrition. But Bernard shows that no such direct nutrition takes place. The proximate principles of the body are formed in the interior of the living organism. It is only their materials which are absorbed from without. The fibrine of the blood, the musculine of the muscles, and the osteine of the bones are not absorbed under that form from the intestine, even when they have been taken as food; but must be reconstructed, after passing through essential modifications.

"In a word," says Bernard (p. 495), "physiologists imagined a direct mode of nutrition, that is, a sort of migration of the proximate principles, ready formed, from the exterior into the body of the animal; and it was a favourite notion to compare this direct nutrition of animals with the different process attributed to vegetables, which were supposed to construct the proximate principles out of ultimate elements. But such an exclusive theory as this cannot be sustained. We have already seen, from what we know of the production of liver-sugar, that it is not necessary for an animal to be supplied with food containing all the proximate principles which go to make up his body. It is undoubtedly necessary that he be supplied with the elements of these immediate principles: but he has the power of modifying them so as to form some new proximate principles out of them. The slightest reflection will convince us

that nutrition does not take place in this direct manner, and that, so far from absorbing passively its proximate principles, the animal organism takes a very active part in their preparation. Not one of the albuminoid elements of the living body, for example, can be absorbed under its own form. It is plain enough that the fibrin and albumen of the blood are not taken up from the intestine as fibrin and albumen. The fatty matters, again, do not exist ready formed in the food; for the solid fats of beef and mutton are not to be found under the same form in the vegetable substances which these animals use as food."

This is certainly the correct and truly physiological view to take of the nutritive process. The elements of the body are undergoing constantly a transmutation, and the new substances are undoubtedly produced, in most instances, by metamorphosis or decomposition, in the very organ or tissue where they first make their appearance. The coagulable matter of the pancreatic juice, for example, which is different from the albumen of the body, is produced in the substance of the pancreas itself. So when the blood is modified in passing through an organ, it is not that its own ingredients are directly changed, but that it absorbs the new substance from the glandular tissue in which it is produced. Thus the sugar of the liver, which is different from the sugar of the food, is formed in the hepatic tissue itself, and absorbed from it by the blood, to undergo further transformation elsewhere; and it will continue to be formed in the liver, though no starchy or saccharine substance be taken with the food. Nutrition, in the animal body, is not a direct and simple process, but an indirect and complicated one.

Bernard, however, draws, as we think, an erroneous conclusion from the above truths. He maintains that animals do not require to be supplied with food containing all the different kinds of proximate principles—albuminoid, oily, and amylaceous—but that they can manufacture the two latter for themselves by the nutritive processes, while the albuminoid substances are the only ones absolutely indispensable. The truth is that, in the herbivorous and omnivorous animals at least, though the *particular kinds* of albuminoid, oily, and saccharine substances peculiar to the body are not necessarily present in the food, yet *substances belonging to each class* are requisite for the proper maintenance of the organism. Carnivorous animals, it is true, may do without starchy and saccharine matters, but they require both fatty and albuminoid substances. Magendie found that dogs fed exclusively on sugar or fat died with symptoms of disordered nutrition; but it has since been found that dogs fed on pure albumen or pure fibrin die just as surely, though after a longer interval. The instinct of the animal revolts after a time at such food, and craves oleaginous substances. In the case of the human subject, an exclusive diet of animal food and fatty matters becomes, after a time, intolerable, and the patient experiences an irresistible longing for vegetable food containing starch or sugar. The albuminoid elements can be dispensed with for a shorter period than the rest, because they form a larger proportion of the entire mass of the body; but the others, even the inorganic substances, are finally indispensable also. A man may be starved to death at last by depriving him of common salt or phosphate of lime, just as effectually as if he were deprived of albumen or oil. The natural instincts, as regards the selection of food, are the only unerring guides in this respect; and we cannot neglect their indications, or replace them by any artificial rules of diet.

The author takes also a somewhat different view of the nutrition of vegetables from that which has usually been received by physiologists. While animals require for their maintenance a supply of proximate principles, starchy, oleaginous, or albuminoid, plants are said to require only the ulti-

mate chemical elements of these principles, or their simplest inorganic compounds. They are said to absorb from the exterior only water, carbonic acid, and ammonia; and are thought to construct out of these materials the cellulose, gum, sugar, and gluten which make up their fabric. It has also been a favourite idea with chemists to indicate a kind of antithetical relation, in this respect, between animals and plants. Plants are regarded as deoxidizers, animals as oxidizers; plants as the fabricators, animals as the destroyers, of organic matter. It is now acknowledged, however, this cannot be regarded as expressing exactly the true relation of animals and vegetables. Animals, as already shown, not only destroy, but also produce particular proximate principles, such as sugar, &c., from materials derived from without. Plants, on the other hand, not only produce and accumulate sugar, gum, and gluten at one period of their growth, but also destroy these substances spontaneously at another.

Bernard maintains also that plants require, as well as animals, proximate principles for their support, and denies that they can flourish when supplied only with water, carbonic acid, and ammonia. It must be confessed, indeed, that the evidences in favour of the opposite opinion, when critically examined, are much less satisfactory than we have been accustomed to think. The vegetable-bearing soil always contains more or less animal and vegetable matter in process of decay; and the more abundant these decomposing substances, the more luxuriant is the vegetation. The habits of certain parasitic plants, which fix themselves upon the bodies of other vegetables or of animals, seem certainly to indicate that they require organic matter in some form for their support. At all events, the question must be regarded as still unsettled.

It is evident, from what has been said, that the nutritive changes which take place in the interior of the body are complex and incessant. The materials which are introduced from without, far from being simply transported and fixed in the animal frame, undergo ceaseless transformations and decompositions, which result at last in their total destruction; the movements of nutritive and destructive assimilation going on together, and giving rise to new products in different parts of the body; the substances which are finally expelled representing the last stage in their progressive metamorphosis.

"It is during these incessant transformations," says M. Bernard, "that those chemical phenomena take place in the organism, which require for their support the concurrence of oxygen. The nature of these phenomena, however, is very imperfectly understood; and though we are acquainted with the two extremes of the process, though we know that oxygen is absorbed and carbonic acid exhaled, that does not give us any information as to the intervening phenomena; any more, to quote the words of an eminent chemist, than we can tell what is going on within the walls of a house, by seeing who goes in and who comes out of it. All these intervening phenomena are, therefore, as yet unknown to us; and it is only by means of physiological experiment that we can ever hope to discover their nature."

The treatise of Bernard *Sur le Pancréas et sur le rôle du Suc Pancréatique*, the title of which also stands at the head of the present article, contains, in a slightly different form, nearly everything on the pancreas and the pancreatic juice to be found in the *Leçons de Physiologie.* It is printed, however, in superior style, and is accompanied with very elegant coloured steel plates, showing the physiological congestion of the pancreas during digestion, the atrophy of the organ produced by injecting it with fat, and the distended lacteals of the rabbit, showing their situation and origin. It is admirably arranged as a monograph, and will long retain its place as the most complete work on the subject.

J. C. D.